stated in the introduction to the volume, the influence of omitted days was duly taken into account, values for such days being adopted from the eye-observations (usually six daily) corrected for diurnal inequality by means of corrections derived from the discussion of the twenty years' photographs. Thus, among the twenty separate daily values on which each mean daily value in Table 77 depends, one or two may be derived from eye-observations in the way described.

The diurnal variation of temperature in the apartment in which the photographic barometer is placed is, on the average, less than one degree. WILLIAM ELLIS

Royal Observatory, Greenwich, October 27

Sun-Spots in Earnest

WITH reference to the fine group of sun-spots to which Prof. Piazzi Smyth draws attention in NATURE, vol. xx. p. 602, it may be interesting to mention that the incipient stage of the group in question is shown on two photographs of the sun taken at the Royal Observatory, Greenwich, on October 16 (two days before the date of Prof. Piazzi Smyth's observation). At that time the group consisted of three "veiled" spots and several very small specks hardly to be distinguished from the ordinary pores, together with small faculæ. No photographs were obtained on the next day, and on October 18 enormous changes had taken place, the "veiled" spots having developed into fine sun-spots, with nucleus and penumbra. Four photographs taken on this day show that changes were still taking place, and these continued throughout the remainder of the period of visibility of the group, viz., till October 21, when it passed off at the west limb. No trace of the group is to be found on two photographs taken on October 15, so that it would appear to have formed between October 15 and 16, and must have been quite in its infancy when first photographed on October 16, being then very nearly on the central meridian.

Several small spots have appeared on the sun lately, but they have been for the most part very short-lived. Thus a group of spots with faculæ, first seen on the east side of the sun on October 15, had completely disappeared on October 16. Another group consisting of six or seven small spots with faculæ, which appeared at the east limb on October 7, had completely closed up in the interval between October 10 and 15. On the whole the Greenwich photographs seem to support Prof. Piazzi Smyth's conclusion that the period of quiescence is now over, and that the solar activity is decidedly on the increase. W. H. M. CHRISTIE

Royal Observatory, Greenwich, October 25

THE Kew solar observations now are, unfortunately, limited to a daily inspection of the sun through a 3-inch telescope, and the drawing of a rough sketch of the spots on its surface, should any be visible, the object the Committee have in view being merely a continuation of the enumeration of the groups as they make their appearance, in the same manner as did Hofrath Schwabe.

I have referred to the sketches drawn on the 15th, 16th, 17th, and 18th instants, in order to see what records they afford of the outbreak of the group of spots mentioned by Prof. Piazzi Smyth in NATURE, vol. xx. p. 602, and find we noted on the 15th two small spots in the sun's northern hemisphere. These were not seen on the 16th, the disk being entered in the register as having "no spots," but at 10.30 A.M. on the 17th a group of small spots appeared to the south of the equator, just in the place occupied on the next day by the group of gigantic spots to which attention has been directed, allowance of course being made for the sun's rotation.

These observations show that the spots did not suddenly burst forth in their full grandeur, but that they broke through the sun's surface gradually, that is to say, the explosion, if such it was, extended over more than twenty-four hours.

In the examination of the Kew solar photograms from 1863 to 1872 now in progress here under the direction of Mr. De la Rue, we have found several instances of similar extensive changes in spots from day to day, not only in the eruption of large spots, but also in their closing up in an equally short space of time.

To give more recent instances, I find that a considerable group of spots was observed on June 28, of which we had no record on the 25th; and again, on July 11, some large spots were noted, whilst on the preceding day, July 10, "no spots" was entered in the register.

The magnetograph curves show a slight disturbance of the magnetic elements on the 16th and 17th, but during the 18th the needle simply recorded its ordinary daily range.

I trust that better-equipped observers will be able to give you more exact accounts of this interesting phenomenon. The sunshine recorder here indicated continuous sunshine on the 16th, occasional gleams on the 17th, and seven hours on the 18th, so the climate cannot be blamed for any shortcomings on the part of southern observers on this occasion. G. M. WHIPPLE

Kew Observatory, October 25

THE conclusion as to the increasing activity of the solar surface, drawn by the Astronomer-Royal of Scotland from his observations of a large solar spot on the 18th instant, is strongly confirmed by the present state of the south-east quarters of the sun's disk. Few prominences are now visible in the other portions of the limb, but on the 26th at 23° 10′ E. of the south point (direct image), the bright line C of the chromosphere extended to the height of 3′ 43″ from the limb, and this morning, the 28th, the greatest height was 1′ 17″ at 18° 46′ E. of S. On the 28th the remarkable prominences extended along the limb from—

18° 8′ E. of S. to 38° E. of S.,

and this morning they were traced from—

10° 51′ E. of S. to 20° 21′.

The ordinary level of the chromosphere does not extend above 5″ from the limb, but to-day it was rather over 6″.

Eight prisms of 60° were used in a Browning automatic spectroscope adapted to an 8-inch achromatic. S. J. PERRY

Stonyhurst Observatory, October 28

Wallace's "Australasia"

ALLOW me to thank the writer of the review in NATURE, vol. xx. p. 597, for some valuable criticisms of my book. It is quite refreshing after the common-place praises of most reviews to have one's errors pointed out and omissions noticed, and I hope to make use of such corrections in a forthcoming new edition. At the same time there are a few points on which I wish to say a word. In the first place the book is not a scientific work, but one of a series intended, as expressly stated, "for general reading." This is, of course, no excuse for errors, but it is a sufficient reason for giving *general* rather than detailed descriptions of weapons, canoes, &c., and for occasionally stating roughly the *size* of an article even when it varies greatly, in order to give definite ideas to readers who may be complete strangers to the whole subject.

I quite agree with my reviewer, that too much is included to be properly treated in one volume, but that was a matter dependent on the arrangement of the series, over which I had no control; and as I had in the earlier portion of the work overrun the space allotted me, I was obliged to restrict my notices of many parts of Polynesia, which is no doubt the most imperfect portion of the volume. It is here that the original work is most utilised, and it will be found that most of the passages criticised (including that in which I am charged with "becoming quite poetical") are Hellwald's. Of course, I should have corrected all his small inaccuracies, but it was almost impossible to do so without rewriting his work altogether. No doubt a very interesting volume could be written on Polynesia alone by the aid of the German authorities referred to by the reviewer; but when I state that the time allowed me for the composition of the entire work was six months, and that I actually completed it in eight, it will be seen that I was compelled to limit myself in the study of authorities as well as in the space I could devote to particular islands.

I think my reviewer forgets the character of the book as essentially geographical, when he objects to my treating New Zealand apart from Polynesia; hence I cannot admit the soundness of his criticism on the comparison of the characters of the Fijians and Polynesians, a comparison which, if I remember rightly, is that of an author who knew them both thoroughly—the Rev. G. Turner. I must also demur to the implication that land can never have extended where there is now a sea 2,000 fathoms deep. I suggest (p. 564) an extension of New Zealand as far as the Kermadec Islands as having possibly occurred "at some remote epoch," and I certainly fail to see its impossibility; yet this is what is suggested by my reviewer's remark, that unfortunately there is a depth of 2,000 fathoms between

them, and that such an extension "cannot therefore have existed." Moreover, the beautiful map of ocean depths with which the volume is illustrated shows a somewhat less depth than 2,000 fathoms on a slightly curved line between the islands, and I believe about the same depth exists between Madagascar and Africa, which have certainly at one time been joined.

There are some other matters touched upon on which I still venture to differ from my reviewer, especially as to the marvellous character of the Easter Island and other remains, and as to the value of the substitution of more for less liberal sectarian teaching in the Sandwich Islands; but on these points I have quoted authorities of considerable weight, and I leave my readers to form their own opinion. As to all matters of fact, I gladly accept correction from one who evidently writes with the advantage of a personal acquaintance with most of the countries referred to in his article. ALFRED R. WALLACE

Climatal Effects of Eccentricity

I AM grateful to Dr. Croll for noticing my letter. But I continue to think that if what seems to me to be the fundamental proposition of his theory, and which I quoted at the beginning of my former letter, be correct, and if the manner in which he and his reviewer have applied it be likewise correct, then we ought to find those differences in the *air* temperatures which my equations indicate. I say air-temperatures, because in Dr. Croll's theory changes of climate are referred to the varying distance of the sun, and climate depends on the temperature of the air.

The heating effect of the sun, other things being equal, has been hitherto assumed to be proportional to the excess of the temperature of the place above the temperature of space. A remarkable result which Pouillet had arrived at, and of which I was not aware when I wrote, shows that this method is incorrect. And I believe that what follows will to some extent afford a reply to the question which I have propounded, and at the same time have a proportionate bearing on Dr. Croll's theory. I quote Pouillet's words from the translation in Taylor's "Scientific Memoirs," vol. iv. p. 83.

"The total quantity of heat which space transmits in the course of a year to the earth and to the atmosphere . . . . would be capable of melting upon our globe a stratum of ice of 26 metres thickness. We have seen that the quantity of solar heat is expressed by a stratum of ice of 31 metres. Thus, together, the earth receives a quantity of heat represented by a stratum of ice of 57 metres; and the heat of space concurs in this for a quantity which is five-sixths of the solar heat. Between the tropics the heat of space is only two-thirds of the solar heat; for the latter is there represented by a stratum of ice of 39 metres."

These surprising results arise from the unequal absorption exercised by the atmosphere upon the heat rays proceeding from the stars and from the earth respectively.

It appears then, that, in applying Dr. Croll's proposition, we ought not to use the value of the temperature of space in forming our proportion, but we ought to use the temperature which the surface of the ground would assume were the sun extinguished. This Pouillet puts at − 89°, or − 128° F. The substitution of 128 for S., instead of 239, reduces my calculated difference between the January and July temperatures at the equator to 11° F., *i.e.*, by about one-half.

If we make the same correction in the case, the high eccentricity at aphelion, for which the *Quarterly Reviewer* has calculated the January temperature of England, and found it 3° F. (I make it even lower), the new temperature comes out 17° F., which can hardly be thought low enough to cause any extreme difference from the present climate. O. FISHER

October 25

THE statement quoted by Mr. Fisher from Dr. Croll (NATURE, vol. xx. p. 577) that "the temperature of a place, other things being equal, is proportional to the heat received from the sun," is based on the assumption of Newton's law of cooling, viz., that the rate of cooling of a body is proportional to the excess of its temperature above that of the surrounding medium. This is approximately true only when the excess is small. When the excess becomes large the rate of cooling augments much more than in proportion. The amount of heat which must be supplied to a body in order to maintain it above the temperature of the surrounding medium is proportional to what would be its rate of cooling. Hence this amount increases as the excess of temperature increases—proportionally while the excess remains small, but much more than proportionally when it becomes large. Conversely, the temperature increases more slowly than the amount of heat supplied, and any variation in the supply will affect the temperature produced in a degree which is less for a large excess than for a small one, and, therefore, less than Newton's rule would give. The excess of the earth's mean temperature above that of space is large, and consequently calculations of changes based on Newton's rule must be in excess of the truth.

The formula obtained by MM. Dulong and Petit (Stewart on "Heat," Art. 235) from the rate of cooling of a thermometer-bulb *in vacuo* makes the necessary supply of heat proportional to $(1{\cdot}0077^t - 1)$, where t is the excess of temperature in Centigrade degrees. If we apply this to the case of the earth, and take 80° F. as temperature at the equator when the earth is at its mean distance from the sun, then the resulting temperatures at its greatest and least distances with our present eccentricity, are given as about 74° and 85° respectively. The fluctuation, which Mr. Fisher makes 21°, is reduced to about 11°. The fall in temperature which would follow a stoppage of the Gulf Stream is made by Newton's rule 59° ("Climate and Time," p. 36): the more accurate formula reduces this to about 37°. Dr. Croll suggests that the temperature of space may be lower than is usually assumed (p. 37). If it be taken as absolute zero (− 459° F.) the fall would not even then come out much greater than 45° F.

Several of Dr. Croll's tables should be similarly modified; at the same time it would be scarcely correct to say that these changes "touch Dr. Croll's theory somewhat closely." They do not invalidate the general contention, that a diminution of the Gulf Stream must diminish the mean temperature of northern regions to a very serious degree. E. HILL

St. John's College, Cambridge, October 25

The Weather and the Sun

PROF. PIAZZI SMYTH in his communication to NATURE, vol. xx. p. 431, evidently infers that changes in the condition of the sun must needs affect every part of the earth in the same way, whereas we have many meteorological analogies, which favour the notion that totally *opposite* effects may arise in different parts of the earth from the action of the *same* primary causes. For example, it is generally assumed that the same tropical heat which gives the primary impulse to the desiccating north-east trade wind of sub-tropical latitudes, furnishes the energy which exhibits itself in the almost constant precipitation under the equator. Any variation in the degree of this heat, should consequently affect localities situated in the region of the trades, and the equatorial calm-belt, in a diametrically opposite manner. Moreover, the notion that the British and Indian rain falls vary together now is altogether inconsistent with the well-known want of similarity between them, both as regards seasonal distribution and annual quantity in the past. It is also remarkable that while the present deluge both here and in India is traced to the sun's "*recovering* his forces and beginning already to shine after his recent languid spotless years with increased radiation on the great oceans of the south," the rainfall of England between latitudes 50° and 55° N. reached a decided maximum in 1877, the year when the sun was, to adopt the favourite metaphor, affected with the most extreme languor, and has been very high all through the period of unusually marked spot minimum, from which we are but just emerging.

The following figures from Mr. Glaisher's reports will illustrate what I have just said.

Great Britain, Lat. 50°—55° N.

Years.	Rainfall in inches.
1875	34·04
1876	34·60
1877	38·55
1878	32·61

More valuable results will generally accrue to science if, instead of founding vague hypotheses on a fancied likeness between isolated weather conditions, at places where the prime meteorological factors act in a totally dissimilar manner; induction is made from results which are derived from trustworthy data, and anticipated by a knowledge of admitted physical principles. As an example of this latter kind, allow me to conclude this letter by exhibiting